



Istituto Sieroterapico Milanese Serafino Belfanti

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Esportazione
CODICI IN USO:
A. B. C. 5TH. & 6TH. EDITION
BENTLEY'S
RUDOLF MOSES

Dear Lederberg,

Thank you for your letter of Dec 26th. I had heard from you of your velvet technique and used a great deal of it in the past six months, with the only modification that, in place of velvet - no sufficiently thickly woven fabrics being available at good prices, I have been using nearly always filter paper (Whatman No. 1 or similar). Filter paper does not seem to transfer appreciable quantities of substances from media to media, and the replicating process is extremely neat. It is mounted on blotting paper pads for ~~the~~ use on large square Petri dishes and on similar gadgets for smaller, ordinary size Petri dishes.

Your application to the demonstration of the mutational origin of resistant individuals seems very elegant. For chloromycetin I had made recourse to a much clumsier, and involved method. It seemed however to have made some effect on Hinshelwood when I told him of it last July. Hinshelwood seems to believe still now that mutants may well be produced on occasions, but that most times a non-mutational adaptation is taking place.

As to "heterothallism": I agree that the use of this word is premature, there being at present no trace of an opposite mating type. The position is however that of the occurrence of self-sterility in a strain, which is on the other hand cross-fertile with ~~all other~~ practically all other K-12 substrains, known to be homo-thallic. The accident must have occurred in the building of Y-10, however, and not in its unbuilding (when TLB₁+ strains were prepared) as ~~this~~ sterility was observed with at least two independent TLB₁+ strains ^(the only ones produced so far) one from Y-10 and one from W 945, when crossed to either Y-10 S^r or to W945 S^r or similar strains. What puzzles me is that after recombination, no trace of sterility is found in the ~~filial~~ offspring; both TLB₁-S^r and TLB₁+ recombinants are fertile



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among themselves and with appropriate TLB₁ parents. A similar loss of a vegetatively stable character had been observed for Hfr. - Even if the use of the word heterothallic is premature, which other word should be used? Incompatible is perhaps less compromising, or rather self-incompatible. Also, I do not see how differential fertility could explain the results; I find little differences of fertility in the various crosses mentioned, except that in those between TLB₁ strains no recombinants have ever been found.

Of course I should be interested to have Mrs. Lederberg's B-M- sterile strain. If, as it seems so far, it ~~did~~^{does} behave as the others, one should think that the mutation involved ~~is~~ as a recurrent one, using Drosophilists' terminology; I have had myself after Ny-treatment a BM- sterile strain, which is now being analysed. I should also be grateful if you would send me strains 58; 679; 679-680. If Mrs. Lederberg's strain is available, could you let me know the story of it.

On crossing TLB₁- filial strains I have found some of the abnormalities which you mention. It seems to me that Y-10 is exceptional under many ways; perhaps some, or all of its peculiarities can be reconduced to one cause.

As to BM-, recovery of prototrophs by mutation does not seem easier on biotin agar. But I doubt that my ^{minimal} media may be slightly toxic, because I have at present difficulties with any back-mutation (but not with recombination!).

Re the Genetics Congress; I know that the Secretary counts to propose your name for a major paper, and that it is planned to give some financial help to all those who will be so asked; but unfortunately the help is bound to be poor. If there is anything in the way of official invitations etc. which we can do from here, and that could help you to provide local financial support please

let me know.

Yours sincerely

Corall